

FROM TEXTS TO PLANTS - OR FROM PLANTS TO TEXTS?

P. J. van Melle

A Difference of Approach, Illustrated by Juniperus chinensis var. globosa Hornibrook, and var. plumosa Hornibr., Dwf. & Slow-growing Conif., 62 and 66 (1923) sensu Cornman and

x J. media var. globosa (Hornibr.) van Melle, and var. plumosa (Hornibr.) van Melle in Phytologia 2 : 191 (1946).

In a thesis: "Studies in the Genus Juniperus" (Cornell University, March 1947) on the taxonomy of junipers cultivated in the United States, John F. Cornman excoriates a paper published by me in Phytologia (as cited), "The Junipers Commonly Included in Juniperus chinensis". His strictures seem excessively voluminous and immoderate, almost obsessive, inappropriate to the usually objective nature of a thesis, and poorly substantiated. He washes my face for me, quite behind the ears, for instance, for my treatment of the above-mentioned Hornibrookian epithets, which, says he, I interchanged, in misunderstanding of taxonomic principle and procedure.

Actually I did no such thing. On the basis of evidence reviewed below I interpret Hornibrook's var. globosa as pistillate. This conclusion is employed by me as a fixed point for the treatment of the wobbly globosa-plumosa complex of Hornibrook, whose 1923 texts appear to me inapt, inconclusive and somewhat confused, while his 1938 texts are more definitely erroneous in important parts. Cornman describes the var. globosa Hornibr. as staminate, and as equalling my var. plumosa (Hornibr.). We cannot both be right.

Of my designation of a Ching No. 53 sheet as the type of my var. plumosa he says that any novice would at once note the discrepancy. I trust that, if he would compare with my designated type sheet (U. S. Nat'l Herb. No. 1245122) unmistakable material of my var. plumosa, the result would fall out more favorable to me. There is no more unmistakable indication of what my var. plumosa is than to describe it as equal (including leaf glands) to Cornman's var. aureo-variegata or my f. albo-variegata - minus the variegations. It equals reverted, green parts of these variants. I reject as inconstant and unreliable the gland character noted by Cornman in his var. aureo-variegata.

If from his point of view I seem to have mistakenly identified my plumosa material with the Ching sheet cited by him he is, of course, free, and in his light bound, to disagree

with me. Such disagreements are no rare occurrence between workers. But it is hardly customary for one to call another practically a nitwit over them or to allege "fundamental error", "misunderstanding of the type method as employed in orthodox taxonomic procedure", etc. It may well be that Cornman, by failing to review my designated type sheet, by reviewing, instead, a sheet not cited in my 1946 paper, and by attaching Van Melle type labels to sheets not so designated by me (pp. 280, 298) permitted himself a taxonomic peccadillo or two.

On p. 291 he says: "Upon inquiry as to why he apparently interchanged Hornibrook's names and descriptions, van Melle writes: 'My treatment of the two varieties represents, in the main, the traditional horticultural grouping. While I base my varietal names on Hornibrook, I did so without any particular deference to Hornibrook's texts'. This violates the basic premise of taxonomy and plant nomenclature, and the conclusions cannot be considered seriously."

I am duly mortified at this public quotation from my correspondence, ungroomed as it was for publication, and looking all the more vulnerable away from its context. My intention was, of course, "the letter of Hornibrook's texts".

From the literature of cultivated plants I gather an impression that a worker's veneration of the letter of texts is proportioned approximately to his own knowledge of his materials. In many cases only that knowledge can provide a proper basis for the evaluation of texts. Without it one becomes easily a slave to the letter of texts - a sort of taxonomic automaton; and there is constant danger of arriving at concepts which, however orthodoxly derived from texts, may bear only a sketchy likeness to existing kinds of plants.

If then, in Cornman's eyes, I seem a little disrespectful of Hornibrook's letter (but does not he, himself, play rather freely with that author's foliage descriptions?), my only defense is that I have had these junipers under observation for over 35 years. My experience with them dates back to 1911, when, at Bobbink & Atkins, Rutherford, N. J. - then large importers - it was my job to check against invoices incoming shipments from Europe and Japan. The junipers under discussion here then constituted a considerable item. The var. plumosa, sensu van Melle, was then coming in from Boskoop mainly as J. japonica, to a lesser extent also as J. chinensis procumbens; the var. globosa, sensu van Melle, as J. japonica globosa, rather rarely as J. japonica nana, and more rarely as J. chinensis procumbens globosa. The name nana was then not in use in Boskoop to an appreciable extent. It was more particularly an English term for the var. globosa, sensu Van Melle.

I am well aware, then, that Hornibrook, in 1923, under

his vars. globosa and plumosa, was not describing theretofore unknown junipers, but was only endeavoring, more or less successfully, to describe kinds that had been long and widely known. Thus, in working over his texts, my principal concern was not: "How can I best preserve the letter of his texts?", but rather: "How can I best manage to present the several elements in this complex correctly and in an orderly grouping?"

Generally, in studies of this sort, my procedure is approximately this: Make an "index plantarum" - an inventory of the elements which require to be accounted for. Enumerate them, even if, at first, only by numbers or letters. Study them; group them as you come to think they should be. Do your own, initial job of taxonomy on them, and give it time to age. Keep correcting, revising, refining. Then compare your work with past treatments. In turn, study this literature; accept from it what revisions or corrections seem in order. But insofar as your own insights continue to appear preferable to you, hold on to them, and do not hesitate to re-interpret, emend or reject past treatments. Trust your own eyes. Finally, express your findings in terms of the available nomenclature. And all this within a not too strict interpretation of the laws.

This sort of thing, says Cornman (pp. 242, 243), is not taxonomy. Unanointed logic! Blasphemy! One can have no truck with it.

Indeed, it is not the way of much of what passes among us for the taxonomy of cultivated conifers. Not all of it, nor even, one hopes, the greater part, but much of it, consists of wholly or largely literary, unrealistic concepts of kinds, derived perhaps through the most meticulous taxonomic procedures from texts and specimens which cannot be identified with known kinds of plants without the aid of extraneous, circumstantial evidence of a chronological or other purely logical nature. It consists on the one hand of ignorance of the living plants involved and on the other hand of a morbid veneration of decrepit texts and specimens.

Some of Cornman's work appears to me to be of this kind. For instance, his J. chinensis, which may be said to occupy a pivotal place in his Thesis, and which ought to represent a fairly concise concept, does not, in my opinion, represent any one definable kind, discerned and then named. It represents, rather, an effort to account for a J. chinensis in terms of the literature of that species - than which there is not a more befuddled chapter in all the taxonomy of junipers. Cornman's var. typica comprises a galaxy of junipers, and a number of obscure literary entities. For instance, of the J. virginica of Thunberg he says: "There is no reason to suppose that he had anything but J. chinensis." One asks:

what grounds are there to suppose that he did have it? And that the J. japonica cernua and dimorpha of Roxburgh represent it?

On pp. 242 and 243 Cornman says of my inclusion in J. chinensis L. of the "variety" oblonga: "In general, van Melle's papers now published must be rejected because of their fundamental errors. He deprecates herbarium specimens as inadequate, as indeed they usually are, but from only a poor photograph of the type of Linnaeus' J. chinensis he attaches Linnaeus' name to what we know as var. oblonga. Thus he rejects customary logic without a trial and depends solely upon circumstantial evidence and 'the resources of disciplined intuition'."

That is a mouthful. In my "Review of J. chinensis et al." (New York Botanical Garden, April, 1947) I admit my inability to identify satisfactorily with the Linnaean type and texts, on a basis of their intrinsic value only, any one known kind of juniper. I admit frankly that my interpretation of the Linnaean record is based for a large part upon the best of chronological and circumstantial evidence at my command. I doubt that Cornman is in a position to do better with it. I regard as the most telling aspect of the specimen the denseness of its (exclusively juvenile) foliage; and of the texts: "Folia....magis quam in reliquis conferta" and "distinctissima densitate foliorum." All this checks very well with the one, distinct, juniper associated by me with J. chinensis: the almost exclusively pistillate J. chinensis foemina of nurseries, originally, and still widely, known as J. chinensis. To its name was added, in the 1850's, the epithet "foemina" to distinguish it from seedlings then being distributed in England by Standish & Noble of the monoecious J. sphaerica Lindl. -- which then, as they do today, bore only or almost exclusively staminate flowers in their youth. These seedlings were at the time erroneously believed to represent the staminate counterpart of the earlier-cultivated, pistillate J. chinensis; and they were called J. chinensis mas or mascula.

Cornman's J. chinensis L. agrees with mine to the extent that he includes the "foemina" material in his var. typica. Only, while I limit my citations to that entity plus its obvious synonyms (including the oblonga name), he presents the var. oblonga as highly distinct. In addition, he cites under his var. typica all manner of other junipers, such as my J. sphaerica vars. dioica and neaboriensis, which are conspicuously different things from the "foemina" material.

This is my view of the oblonga name: It was given in 1914 by Bobbink & Atkins to an importation from Europe, while I was in their employ. It was my judgement then, and has been ever since, that the material so named differed in no manner



worth recognition from that which had been theretofore known at Bobbink & Atkins and elsewhere as J. chinensis and J. chinensis foemina. It is very well possible that, at the time, it appeared to somebody as a little different in general aspect - in the way that many a batch of the "foemina" material, perhaps pruned a little differently, or grown on a different soil or under different conditions, looks a little different from another. Comparisons at the time, and since - many of them based upon large plants distributed under the two names - convince me that the oblonga listing represented nothing but a new name for a very old thing. Probably no juniper has been sent into the world under more different names than the old "foemina" material.

Is there anything about my disposition of this name to warrant Cornman's allegation of "fundamental error" or "rejection of customary logic without trial"?

But, to return to the vars. globosa and plumosa Hornibr. - my first move toward a treatment of this complex was the making of an inventory of the elements in it, known to be in cultivation. This inventory revealed the existence of the following two groups, a and b:

Group a, consisting of  
2 elements,

both pistillate:

a, the typical green form

a 1, a yellow-suffused form

Group b, consisting of  
4 elements,

all staminate:

b, the typical green form

b 1, a yellow-suffused form

b 2, a yellow-variegated form

b 3, a white-variegated form

My index did not include the globosa cineria element described by Hornibrook in 1923 from a small plant, now extinct. A corresponding element was known in England about 1910 as nana glauca. It does not appear to have become widely grown in Europe. Again, in 1940, Grootendorst described a J. chinensis Blaauw's Variety (possibly the same element) as a novelty from Japan, about to be introduced in Europe. The illustration of this plant suggests the habit of my staminate group b.

The idea of these two, apparently closely related groups, one pistillate, the other staminate, may look artificial. Indeed, the fact that no staminate seedling of my pistillate group appears to have been known is noteworthy. It is matched in other kinds of juniper. None is known of my x J. media var. arbuscula, of J. procumbens Sieb. & Zucc., or of J. squamata var. Meyeri Rehd. The last recorded, cultivated material of the J. chinensis foemina of nurseries (J. chinensis L. sensu van Melle) is represented by the f. aurea (Young), which originated in England before 1872. Yet pistillate plants of these several kinds fruit freely in nurseries.

I do not propose to explain the phenomenon. I merely report it. It seems probable that apomixy is involved. The matter merits investigation. At any rate, I need not hesitate to present my groups a and b, respectively, as pistillate and staminate. In each of them the constituent elements are mutually identical except as to color, and for a not consistent tendency in a 1 and in b 1 and 2 toward protracted juvenility. Neither do I hesitate to reject Cornman's presentation of his var. aureo-variegata as intermediate between my two groups.

My pistillate group a equals the materials shipped into the United States from Boskoop in large quantities as J. japonica globosa and globosa aurea; my staminate group b, those shipped in from Boskoop mainly as J. japonica, japonica aurea, aureo-variegata and albo-variegata, and to a lesser extent by corresponding chinensis procumbens listings.

It seems that Hornibrook's globosa group (including the misplaced f. cineria) was matched in England by a group of three nana listings. An R. H. S. Award of Merit was given on December 10, 1908 "To J. chinensis nana aurea from Messrs. Waterer, Bagshot. Three new dwarf forms of the Chinese juniper were shown - nana, nana glauca and nana aurea."

It seems certain that both the globosa and plumosa groups were included in Beissner's J. chinensis procumbens of 1891, and in the notoriously inclusive japonica listings as far back as the early 1870's - at which time, also, the Pfitzer Juniper made its debut, in France, as J. japonica pendula.

The first clear separation of the two groups discernible to me is that of the Boskoop listings in the early 1900's of J. japonica, with 3 variants, and J. japonica globosa, with 1 variant. However, it is on the more or less formal literature of their names that one must base one's choice of names. Therefore, Hornibrook's texts merit consideration. Yet, if it were not that his names have become established in horticulture I might well have rejected them as nomina dubia. For I see in them very little literal, descriptive, distinguishing value. I preferred, however, to explore what sense might be discovered below their textual surface, and from the related nana listings of 1923. Thus I discovered implications more compelling than the letter of the texts.

Except for the aureo-variegata and albo-variegata elements, my treatment of the complex is based, nomenclaturally, upon Hornibrook's 1923 edition. I need not accept the 1938 treatment, even though my grouping of the variants and my reduction to synonymy of the var. decumbens with var. plumosa, and my citation, under the latter, of the J. japonica and J. chinensis procumbens of Boskoop concur with the 1938 edition.

While Hornibrook nowhere states outright the sex of the

materials except that of his globosa cineria, he says in 1923 that his var. globosa and f. aurea are of the same sex; that they have not yet borne fruit, while "form 3 (cineria) is a male plant." Again, in 1938: "Var. globosa cineria is a different form; it is a male plant." Does not this imply that his var. globosa and its aurea form were pistillate?

In 1923 he lists a var. nana (Hochst.) as follows: "A form received from the Arnold Arboretum without description. Extremely slow-growing....it would seem to be the erect form of the var. globosa, its sprays being similar to that variety in size and general appearance." Living plants at the Arnold Arboretum harking back to material received there as var. nana, from Waterer in 1909, and from Farquhar in 1917, are all pistillate, identical with my a element - with my var. globosa. They do not differ from it by any supposed erect character, by which, only, Hornibrook distinguished this material from his var. globosa. The type specimen of my var. globosa was taken from the Farquhar material, even though this is labelled var. plumosa in the Arboretum.

I think that I am on safe enough ground in identifying Hornibrook's globosa group, consisting (exclusive of the misplaced cineria form) of 2 elements, with my pistillate group a, which consists of 2 elements. Having done this, - then, if Hornibrook's plumosa group (consisting of 4 elements) is to be interpreted at all in terms of the known junipers within the complex, I am bound to identify it with my staminate group b which consists of 4 elements. In the case of two of Hornibrook's four plumosa elements - his aureo-variegata and albo-variegata - anyone may ascertain for himself that these (the only known variegated elements in the complex) are both staminate. Since nothing in the 1923 edition militates otherwise, I assume, as a matter of simple logic, that Hornibrook's plumosa aureo-variegata and albo-variegata, minus their variegations, equal his var. plumosa. Such is the composition of my plumosa group; b 2 minus 2 equals b; b 3 minus 3 equals b.

This is no over-simplification. Thus, my treatment of the globosa-plumosa complex is reduced to the grouping of its several elements under a pistillate and a staminate type, as shown in my a and b schedule. Each group has its consistent, distinct characters. All that is needed to place any of the elements under its proper heading is to know either its sex or its habit. My Latin diagnoses attribute to the pistillate and to the staminate groups the characters that go with them. This involves no undue violence to Hornibrook's 1923 texts. But it is at variance with the habit ascribed by him in the 1938 edition to the J. japonica and J. chinensis procumbens of Boskoop - names which he associates with his var. plumosa. I quote: "As grown in large quantities in Holland,

this form (plumosa) makes eventually a somewhat loose shrub about twice as broad as high.....Hitherto this form has been sent out by well-known Dutch nurseries under the names J. japonica and J. chinensis procumbens." This is not the habit of these two Boskoop listings, but that of the J. japonica globosa of Boskoop - my var. globosa (Plate X in my "Review"). A true, youthful habit phase of the J. japonica of Boskoop is shown by Hornibrook under his var. globosa, in the Report, Conf. Conference (1932) and in his 1938 edition. This is the habit of the staminate material - not that of the var. nana, not of his var. globosa of 1923.

I conclude that Hornibrook at no time appeared to know clearly, one from the other, the green types of his vars. globosa and plumosa. Had he understood them well, he would not have been as uncertain as he appears to be in 1938 about where to place his variegated forms. I quote: "On the whole they approach nearest in habit and foliage to J. c. var. plumosa Hornibr.....and possibly the best way out of the existing confusion is to classify them as forms of var. plumosa; the suggestion to do so originates with Mr. Herman J. Grootendorst of Boskoop, whose firm has grown and observed these forms for a great many years, and on consideration I agree with him." Actually, these staminate forms which he places with his var. plumosa are variants of the material depicted by him under his var. globosa. No careful worker, aware of these defects in Hornibrook's post-1923 treatment would wish to perpetuate them; and it is because of them that I exercise my right to reject all or part of the post-1923 treatments.

Cornman follows me in associating the habit "ultimately twice as high as wide" with the pistillate sex; but, unaware of Hornibrook's mis-description of the J. japonica of Boskoop and of the implied pistillate nature of the 1923 var. globosa, he reverses my names. To his variegated material he ascribes, erroneously, the habit of the pistillate and the sex of the staminate group, as well as a gland character different from that which he notes under his vars. globosa and plumosa.

In terms of the known elements in the complex his var. aureo-variegata is neither flesh nor fish. There are no such intermediate elements; and the glands in the variegated material are as variable as they are throughout the complex. Cornman sidesteps the impasse of his aureo-variegata material by saying: "There is no compulsion to classify it nomenclaturally as a variant of either" variety.

I believe that in retaining the grouping of my a and b schedule, in line with the usage of Boskoop and with Hornibrook's nomenclatural grouping, I have preserved a helpful perspective, which is lost in Cornman's treatment. I believe



that my investigation of Hornibrook's treatments is at least as thorough as Cornman's, and more closely related to the living materials involved; and that Cornman, having failed to discover the implications and defects noted by me, was at a disadvantage, and in no position to spank anybody.

He errs, also, in lumping with his var. aureo-variegata the albo-variegata element, on a basis of preserved specimens. I should not dare do such a thing, knowing that white variegations of junipers often turn to various pale to deep shades of yellow on herbarium sheets, in a short or longer period of time. Actually, the albo-variegata material differs from the aureo-variegata both in the color and in the size of the variegated parts. The effect (as Hornibrook notes correctly) is a speckled one; - like that of J. Sabina f. variegata.

Then, having thrown out Hornibrook's albo-variegata listing, he proceeds to transfer its synonym, J. chinensis procumbens albo-variegata of Beissner (1891), to the var. alba Rehd. But I shall not go into a detailed criticism of Cornman's dispositions. I have limited myself to two of the numerous instances in which his sweeping condemnation of my work appears wholly out of order. It would have been unreasonable to expect his thesis on cultivated junipers to be a work of mature skill. It is an initial effort. If he is to do effective work in this field, he may well arrange to swap certain apparent mental attitudes for new ones.

For instance, he may well cultivate a more critically inquisitive mind in relation to problems of the origins of many of these materials. To dismiss them, with a sort of finality, as "garden forms" and "clons" seems a facetious and unscientific way of disengaging oneself from one of the most important and difficult inquiries concerning them. It is a charming, but unrealistic thought that such things as "J. chinensis" vars. pyramidalis, Sheppardii, Parsonsii, Pfitzeriana, etc., are found in horticultural cabbage patches.

On p. 244 he says that all the cultivated varieties of J. chinensis, except var. Sargentii, "so far as has yet been shown, are apparently variants selected from cultivated plants; most of them are clons." I object particularly to the word "apparently". For instance, in the case of his var. Sheppardii (my J. Sheppardii), of which we have in cultivation monoecious material and dioecious, in both sexes, and a diversity of color forms, I do not believe that it can be "apparent" to any able observer that this aggregate represents a variant selected from any cultivated kind, let alone, directly or indirectly from the strictly dioecious "foemina" material. This goes as well for J. sphaerica Lindl., of which clearly and obscurely monoecious as well as dioecious

phases and varieties, in both sexes, are cultivated. The Pfitzer Juniper - a selected variant? Of what?

Cornman criticizes me sharply for identifying some of my junipers with wild materials. I can see nothing but prejudice in that attitude. In the case of Japanese specimens cited by me for my J. Sheppardii var. torulosa (Eastw.), he does not review these sheets, but passes them by with the brief, erroneous observation that all are from Honshu, and none unquestionably wild. Yet they include Wilson's collection from Yakushima, reported by him as wild, and another of his collections from the Idzu Peninsula, which he reports as "said to be wild". Inasmuch as herbarium specimens of this variety are mostly very clearly recognizable, I suppose that Cornman would have to admit them as identical with the cultivated material of torulosa, if he were to account for them. In the cases of the Sheppardii and sphaerica entities, he ignores the occurrence in cultivation of monoecious materials. I believe that I could show him cultivated plants of J. sphaerica of every degree of monoecism. The thought seems to be repulsive to him that among the so-called varieties of J. chinensis there should be perfectly good wild species and varieties; and he leans over so far in this prejudice that in a number of instances his judgement seems seriously impaired. While he accounts faithfully for every monoecious species published as such, for instance, by Martinez and Florin, he jabbars about the monoecism of J. sphaerica Lindl. representing, perhaps, only an abnormality of the type specimen, while it is abundantly manifest in cultivation.

It seems to me that in relation to the origins of cultivated junipers we must either bury all thought under platitudes such as that cited from Cornman's thesis (p. 244), or else explore every avenue of inquiry. I can see no valid objection to the traditional practice of checking cultivated materials of unknown origin against herbarium records of wild plants, which is what I have done. That, at least, represents a mode of inquiry into the origins of cultivated plants. The opposite of it is Cornman's refusal to admit the wild nature of Faurie's specimens of J. procumbens Sieb. & Zucc. Bound to diagnose it as a cultivated variety of J. chinensis, he suggests that Faurie may have had his notes mixed up.

There is, I think, no more justification, in the absence of positive evidence, for ascribing to any of these junipers a garden origin than for declaring them to be wild. The one assumption is as prejudiced as the other. Yet, I think it is perfectly legitimate to speculate and conjecture about their origins, as long as conjectures are clearly presented as such.

If Cornman is to do effective and intelligible work on

our cultivated junipers I believe that he will have to do a sorting-out job on his J. chinensis, which is, as yet, in my opinion, a loosely inclusive, undefinable concept, based not upon observation of living plants, but upon the literature of what he probably believes to be that of J. chinensis, but which, from the 1850's on, is largely that of J. sphaerica. He is more likely to arrive at clarity in the matter from the study of living plants than from the literature.

I regret that his initial contribution becomes for me the occasion of this rebuttal. I congratulate him upon his ordination, and trust that he will increase in wisdom and in stature; that he will come to contribute much to the knowledge of cultivated junipers. He is as yet over-dependent upon texts. I hope that he may come to work increasingly from living plants toward texts, and bring with him into this field a refreshing breeze of first-hand knowledge of the living plants. That is what is mostly needed.

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### NOTES ON NEW AND NOTEWORTHY PLANTS. III

Harold N. Moldenke

ALOYSIA VIRGATA var. ELLIPTICA (Briq.) Moldenke, comb. nov.

Lippia virgata var. elliptica Briq., Ann. Conserv. & Jard. Bot. Genev. 7--8: 304. 1904.

This was published as "var. platyphylla" through typographic error in Phytologia 2: 310. 1947.

BUDDLEIA MEGALOCEPHALA f. ALBILANATA Moldenke, f. nov.

Haec forma a forma typica speciei tomentis albis recedit. -- This form differs from the typical form of the species in its very dense tomentose pubescence on the branches, branchlets, petioles, lower leaf-surfaces, peduncles, and calyxes being white.

The type was collected by Jacob F. Brenckle (no. 47-283) at high altitudes in the pine barrens east of Lake Atitlan, Guatemala, on February 21, 1947, and is deposited in the Britton Herbarium at the New York Botanical Garden. The collector describes it as a thick and short tree, the old trees leaning over and supporting on their trunks epiphytic ferns, orchids, mosses, etc.

CITHAREXYLUM ULEI var. CALVESCENS Moldenke, var. nov.

Haec varietas a forma typica speciei foliis subtus non hirtellis recedit. -- This variety differs from the typical